Letters to the Editor.

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The International Research Council.

THE issue of the *Times* published on March 8 contains an article headed "The Progress of Science: Revolt against Super-Organisation." A few words of comment are necessary, though the task is disagreeable owing to the general tenor of the article, which in parts is frankly abusive and in others misleading. Its chief invective is directed against the International Research Council. This, according to the author, is to be "the supreme body in all the affairs of science," and he follows up this product of his imagination by enumerating in the same sentence the avowed objects of the International Research Council, placing a pure invention of his own in juxtaposition to the actual functions of the body concerned so as to leave the impression that both have equal authority.

The International Research Council was founded in the first instance through the action of the Royal Society and the Academies of Paris, Italy, Brussels, and Washington. Its object was to reorganise international work which had come to a standstill through the war, and to extend it where found desirable. The question as to the time at which former enemy countries should be admitted is a matter for argument, and it may be the policy of the *Times* to urge their immediate inclusion in the interests of the general peace of the world. Recent incidents at a meeting in Paris at which a German professor took part do not confirm this view, but the question has really nothing to do with the purpose which the article pretends to discuss. It should not be forgotten, however, that a friendly personal intercourse is an essential condition of the success of international conferences. This is recognised by the countries neutral during the war, which have nearly all accepted the invitation of the International Research Council to take part in this common enterprise.

The International Research Council has initiated the formation of unions for the conduct of scientific work. In the subjects of astronomy, geodesy and geophysics, and chemistry such unions are actually at work, and two others have been formed. Once an international union is established it becomes autonomous, and conducts its work without interference from the International Research Council except in a few matters in which a common policy is desirable.

Everyone knows that the decisions of an international conference are only advisory, and have no binding force on the separate countries. Representatives taking part in the conference report to the home authorities concerned, who act as they think fit, accepting, no doubt, in general such recommendations as have secured practical unanimity. At a recent meeting in Brussels certain countries desired to initiate the formation of an International Union of Biology, and their representatives tentatively drew up some statutes. These were submitted to a competent body in this country, which reported unfavourably, and there the matter ends so far as Great Britain is concerned. This does not, of course, prevent France, Italy, the United States, and other countries from forming a Union of Biology if they wish. I fail to understand where the grievance of the *Times* comes in.

ARTHUR SCHUSTER,
General Secretary of the International
Research Council.

The Constitution of the Alkali Metals.

IN a recent letter (NATURE, February 24, p. 827) attention was directed to positive rays of metallic elements generated by means of a heated anode by which lithium (atomic weight 6·94) was demonstrated to contain two isotopes 6 and 7. The mass spectrograph has now been successfully applied to the analysis of these rays and the investigation thereby extended to the heavier members of the group.

The method presents some peculiar technical difficulties, and the intensity of the lines yielded is very poor in comparison with that of the "gas" lines produced by the ordinary discharge tube. On the other hand, the arrangement is such that *none* of these ordinary "gas" lines appear at all, so that any line, however faint, if satisfactorily confirmed by repetitions with different fields, is conclusive evidence of the presence of metallic atoms of corresponding mass in the salt employed on the heated anode.

Sodium (atomic weight 23·00) is the easiest metal to deal with; its mass spectrum consists of a single line only. From the known values of the fields employed this line is in the position expected from the atomic weight; it is therefore assumed to be exactly 23, and used as a standard comparison line.

Potassium (atomic weight 39·10) gives a strong line at 39 and a very weak companion at 41. These figures are integers within about a quarter of a unit when compared with sodium 23. The relative intensities of the lines are not inconsistent with the accepted atomic weight. Potassium therefore probably consists of two isotopes 39 and 41.

Rubidium (atomic weight 85·45) gives two lines two units apart of relative intensity about 3 to 1. Comparison with the potassium line 39 gives these the masses 85 and 87 to within a fraction of a unit. As these values are in excellent agreement with the accepted atomic weight they may be taken, provisionally at least, as the weights of the two isotopic constituents of rubidium.

The mass spectra obtained from cæsium (atomic weight 132·81) have so far exhibited only one line, which when measured against the rubidium lines indicates a mass 133. The intensity of this line leaves much to be desired, but it is sufficient to point to the conclusion that if, as the atomic weight would lead one to expect, another isotope of cæsium exists, it is present in proportions of less than 5 per cent.

F. W. ASTON.

Cavendish Laboratory, March 12.

The Designation of Vitamines.

IT is often said that a rose by any other name would smell as sweet but in chemistry this is not the case; the name is of consequence and the choice limited. I am glad that Prof. Liversidge takes exception in NATURE of March 10 to the sufficiency of the suggested dropping of the "e" from "vitamine"—the sting is still left in the "amin" tail; moreover, the word should be got rid of altogether, as it is but a monument of a gross experimental blunder.

In my early days one of the most valuable lessons I learnt was from the late Prof. A. W. Williamson, one of the keenest intellects of his day among chemists. He always insisted that we did well to use non-committal names—names which did not give expression to a view open to question but were simply descriptive of some recognisable character in no way open to doubt. No better illustration can be given than the use of the name "carbamide" for urea, actually enforced by the Chemical Society. "Urea" is non-committal but absolutely significant of the

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origin of the substance, nothing more; "carbamide" is suggestive of a particular structure, of a view which, so long as I can remember, has not been in accordance with the facts and is now, I suppose, fairly generally abandoned, though the error is still perpetuated in the text-books—but one of the main purposes text-books serve is the perpetuation of error. Other cases might be quoted; time was when "constitutions" were settled on paper and not a few names are survivals of the practice.

In a course of Cantor lectures on "Food Problems" which I gave in May, 1919, I suggested the use of the term "advitant" in place of "vitamine." A word of good clang, its meaning is clear and will be obvious to most; the substances it is intended to cover *are* necessary to life and we may as well say so, though we have not the faintest idea what they are.

HENRY E. ARMSTRONG.

Relativity and the Velocity of Light.

MR. BARTRUM's excellent letter on p. 42 of NATURE of March 10 has done good service in extracting an explanation from Dr. Jeans, but the latter will forgive my saying that his position is not clear yet, at least not clear to me. Briefly thus:

(*a*) If we are able to compare the velocities of two single light journeys, one of which may be under normal conditions and therefore known, surely we have determined the other.

(*b*) I cannot see that Majorana's interesting experiments prove more than that the propagation of light has the characteristics of wave, and not projectile, motion.

(*c*) I admit Dr. Jeans's equations (1), (2), and (3), but I can see no merit in afterwards introducing v. If they are true for all reasonable values of u, what more is gained by writing $u+v$ instead of u? Are they not the same thing?

OLIVER LODGE.

The Peltier Effect and Low-temperature Research.

FROM certain considerations emphasised by me in the *Phil. Mag.* for December (Supplement), 1876, especially § 33, ser. v., vol. ii., p. 538, about true contact e.m.f., I concluded that such forces are intimately connected with electrical resistance; good conductors fail to get a grip on the electricity so as to propel it effectively, while the grip of insulators is tremendous. Consequently it is probable that at any temperature at which electric resistance ceases, the Peltier effect will cease also.

OLIVER LODGE.

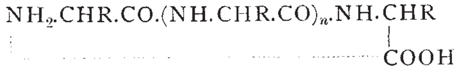
The Nature of the Emulsoid Colloid State.

THE publication in the Transactions of the Chemical Society for December last of the latest of the extremely valuable and interesting investigations by Prof. J. W. McBain and his collaborators on soap solutions leads me to direct attention to a hypothesis as to the nature of the emulsoid colloid state which I have briefly indicated in a technological paper on "Colloidal Fuels" (*Journ. Ind. Eng. Chem.*, vol. xiii., p. 37, 1921). The stabilising colloids used in these fuels belong to a class of bodies forming emulsoid sols and gels in *non-aqueous* systems, *e.g.* in hydrocarbon oils. The parallelism between certain such non-aqueous systems and aqueous emulsoids has struck many observers (notably M. Fischer) and caused considerable doubt as to the validity of the application of ionisation theories to the emulsoid colloids. Certainly it would appear that any theory of the emulsoid systems must explain why sodium oleate forms sols and gels with water, whereas aluminium oleate does so with benzene.

The theory of micellar orientation supported by McBain appears to suffer in this respect, that the "micelle" postulated is already a micro-colloid system, and the colloid properties are already present in the "micelle." A consistent theory of emulsoids must not only be in agreement both with the physical properties (viscosity of sols, gelation, elasticity of gels, hysteresis, etc.) and with the facts as to chemical constitution (polysaccharide character of starches and celluloses, polypeptide character of proteins, fatty acid salts for soaps, etc.), *but it should also show the physical properties developing from the chemical composition and constitution.* If colour and selective absorption flow from chemical composition and constitution, there appears no reason why cohesion and selective adsorption should not do so also. The suggestion which I put forward was stated as follows (*loc. cit.*, p. 42):—"Such gels—(heat reversible) not coagula—may be imagined as very tenuous web-works, or foams, the mesh or walls of which are very probably sub-molecular in dimensions, or the whole mass of the colloids forms one 'molecule' uniformly dispersed through and partially dissolving the solvent. By partially I mean that only part of the 'molecule' of the emulsoid is consolute with the solvent or dispergent, while the other part of it is insoluble, and its atoms tend to unite, forming a semi-rigid framework."

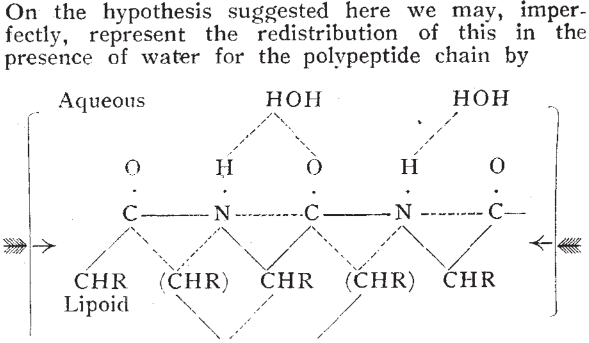
The hypothesis proposed does not regard micellar orientation (and attraction) as primarily responsible for emulsoid sols and gels, but rather sub-molecular (or transmolecular) orientation of definite atom-groups, entirely in the sense of the theory of molecular orientation due to structure proposed for surface and interfacial tension phenomena by W. B. Hardy (Proc. Roy. Soc., vol. lxxxvi., A, p. 610, 1912), J. Langmuir (Journ. Amer. Chem. Soc., vol. xxxviii., p. 2221, 1916; *ibid.*, vol. xxxix., p. 1848, 1917), and W. Harkins (Journ. Amer. Chem. Soc., vol. xxxix., pp. 354 and 541, 1917).

The genesis of a micelle, as plurimolecular unit of a colloid system, may be regarded as a consequence of equilibrium, usually incomplete, between homochemical solution forces and heterochemical forces, the former tending to dissociate and decompose the chemical molecule, the latter resisting decomposition. In the case of proteins the most probable general type of linkage, according to H. A. Plimmer ("Chemical Constitution of the Proteins," part ii., p. 2), is of the form



$$\begin{array}{r} NH_2.CHR.CO.(NH.CHR.CO)_n.NH.CHR \\ | \\ COOH \end{array}$$

where n refers to the degree of polypeptide condensation and R is an alkyl or other substituent group. On the hypothesis suggested here we may, imperfectly, represent the redistribution of this in the presence of water for the polypeptide chain by



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